intercourse of different branches of the human race? That nations should think or feel exactly in unison is no more to be expected than that individuals of the same family should possess identical tastes and habits; but as in the smaller, so in the larger groups, these distinctive characters may and ought to exist apart from every unkind jealousy or envious bitterness. There had been far too much of this in past days, and we hail with pleasure the appearance of this friendly book, which has evidently been drawn up in a truly kind and genial spirit.

If it puts us somewhat to shame, that the Assistant-Astronomers of the Paris Observatory should be telling us what goes on at our own doors, we have only ourselves to thank for the omission, and them for the way in which they have supplied it. The plan they have adopted is an excellent one; and as to its execution there is very much to praise. A history of English observatories and their work could not be otherwise than somewhat unequal in its execution; it would probably be so to some extent even in native hands; to a foreigner, who must, generally speaking, depend upon communicated information, the difficulty would be insuperable; and to this cause we may evidently refer the omission of some finely appointed private observatories, such as those of the Rev. H. C. Key, with its 18-inch silvered speculum, of Mr. Bird, the Rev. E. L. Berthon, Capt. Noble, Mr. Neison, Mr. Barnes, and many others. It is, in fact, in these private "telescope-houses" that England is so rich, as was formerly remarked in substance to the present writer by M. Léon Foucault, and it is through their work that much of the physical astronomy of the day has been advanced to its present position. But this is exactly what would escape the notice of any but ourselves, and even the generality of ourselves; and in this respect there is, of course, a good deal of deficiency in the work which cannot well be blamed. But great pains have evidently been taken to insure correctness, and to impart knowledge which to many among ourselves will have all the interest of novelty; and this has been done, for the most part, as far as we can judge, in a very satisfactory way.

The French language is now so generally understood among us that a translation is perhaps not required; but should it be undertaken, or should the authors, as we hope, be encouraged to send forth another impression, we would request permission to offer a few suggestions. The Bedford Catalogue, which has had so marked an influence on English astronomy, would well come in for a share of notice: the names of the opticians, whose work is described—which are seldom given, and perhaps not always correctly—might be supplied with advantage; several orthographical slips, and one considerable error in the little map, might be rectified. With these improvements, and some difference in the arrangement and appropriation of the very pretty illustrations, this charming little volume, even now greatly to be commended, would meet our expectations in every way. T. W. W.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. No notice is taken of anonymous communications.*]

Quantitative Relations of Cause and Effect

AFTER Mr. Spencer has implied that he will not himself continue the controversy further, Mr. Hayward, in his last letter, has confused the issues by misstating Mr. Spencer's position. In the circumstances, perhaps, it will not be improper for me, as one familiar with Mr. Spencer's psychological doctrines, to rectify Mr. Hayward's error and explain that which he misapprehends.

The cue may be taken from an experience described in Mr. Spencer's "Principles of Psychology" (§ 468, note), where it is shown that when with one hand we pull the other, we have in the feeling of tension produced in the limb pulled a measure of the reaction that is equivalent to the action of the other limb. Both terms of the relation of cause and effect are in this case present to consciousness as muscular tensions, which are our symbols of forces in general. While no motion is produced they are felt to be equal, so far as the sensations can serve to measure equality; and when excess of tension is felt in the one arm, motion is experienced in the other. Here, as in the examples about to be given, the relation between cause and effect, though numerically indefinite, is definite in the respect that every additional increment of cause produces an additional increment of effect; and it is out of this and similar experiences that the idea of the relation of proportionality grows and becomes organic

A child, when biting its food, discovers that the harder he bites the deeper is the indentation; in other words, that the more force applied, the greater the effect. If he tears an object with his teeth, he finds that the more he pulls the more the thing yields. Let him press against something soft, as his own person, or his clothes, or a lump of clay, and he sees that the part or object pressed yields little or much, according to the amount of the muscular strain. He can bend a stick, the more completely the more force he applies. Any elastic object, as a piece of india-rubber, or a catapult, can be stretched the farther the harder he pulls. If he tries to push a small body, there is little resistance and it is easy to move; but he finds that a big body presents greater resistance and is harder to move. The experience is precisely similar if he attempts to lift a big body and a little one; or if he raises a limb, with or without any object attached to it. He throws a stone: if it is light, little exertion propels it a considerable distance; if very heavy, great exertion only a short distance. So, also, if he jumps, a slight effort raises him to a short height, a greater effort to a greater height. By blowing with his mouth he sees that he can move small objects, or the surface of his morning's milk, gently or violently according as the blast is weak or strong. And it is the same with sounds: with a slight strain on the vocal organs he produces a murmur; with great strain he can raise a shout.

The experiences these propositions record all implicate the same consciousness—the notion of proportionality between force applied and result produced; and it is out of this latent consciousness that the axiom of the perfect quantitative equivalence of the relations between cause and effect is evolved. To show how rigorous, how irreversible, this consciousness becomes, take a boy and suggest to him the following statements:—Can he not break a string he has, by pulling? tell him to double it, and then he will break it. He cannot bend or break a particular stick: let him make less effort and he will succeed. He is unable to raise a heavy weight: tell him he errs by using too much force. He can't push over a small chest: he will find it easier to upset a larger one. By blowing hard he cannot move a given object: if he blows lightly he will move it. By great exertion he cannot make himself audible at a distance: but he will make himself heard with less exertion at a greater distance. Tell him to do all or any of these, and of course he fails. The propositions are unthinkable, and their unthinkableness shows that the consciousness which yields them is irreversible. These, then, are preconceptions, properly so called, which have grown unconsciously out of the earliest experiences, beginning with those of the sucking infant, are perpetually confirmed by fresh experiences, and have at last become organised in the mental structure.

It is not, however, any such experiences which Mr. Hayward adduces to exemplify organic preconceptions. He asserts that his "principal 'exemplification of unconsciously-formed preconceptions' was of Mr. Spencer's own choosing, namely, Newton's 'Second Law of Motion.'" This is an error: Mr. Spencer gave *no* examples of unconsciously-formed preconceptions. If Mr. Hayward will refer to Mr. Spencer's letter in NATURE, vol. ix. p. 462, he will find that Mr. Spencer has described the unconsciously-formed notion of the relation between cause and effect in general terms, and without example or illustration. In his last letter he simply named the relation between muscular tensions and their effects. Probably he expected Mr. Hayward to seize his meaning without any specific example.

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The examples given by Mr. Spencer were examples of *consciously*-formed conceptions based on this *unconsciously*-formed preconception acquired during childhood and boyhood. Mr. Spencer gave three instances into which this preconception tacitly enters: one chemical, another relating to the melting of ice, and a third to the process of weighing. The last is the only one into which the relation between force and motion can be supposed to enter. But the consciously-formed conception that double weights will balance double masses, and so on, is not one into which there really enters any relation between force and motion. The notion of weighing is that of the equal forces of equal masses at the ends of equal levers. So long as there is motion, there cannot be equilibrium. The idea of motion is excluded when weighing is complete.

When Mr. Hayward says that Mr. Spencer has taken Newton's "Second Law of Motion" as an example of unconsciously-formed preconceptions, he utterly misapprehends Mr. Spencer's meaning. The "Second Law of Motion" is one of those developed *conceptions* derived from the organic *preconceptions* above described.

Mr. Spencer's argument appears to be briefly this:—1. There are numberless experiences unconsciously acquired and unconsciously accumulated during the early life of the individual (in harmony with the acquisitions of all ancestral individuals) which yield the preconception, long anteceding anything like conscious physical experiments, that physical causes and effects vary together quantitatively. This is gained from all orders of physical experiences, and forms a universal preconception respecting them, which the physicist or other man of Science brings with him to his experiments.

2. Mr. Spencer showed in three cases—chemical, physical, and mechanical—that this preconception, so brought, was tacitly involved in the conception which the experimenter drew from the results of his experiments.

3. Having indicated this universal preconception, and illustrated its presence in these special conceptions, Mr. Spencer goes on to say that it is involved also in the special conception of the relation between force and motion, as formulated in the "Second Law of Motion." He asserts that this is simply one case out of the numberless cases in which all these conscious-reasoned conclusions rest upon the unconsciously-formed conclusions that precede reasoning. Mr. Spencer alleges that as it has become impossible for a boy to think that by a smaller effort he can jump higher, and for a shopman to think that smaller weights will outbalance greater quantities, and for the physicist to think that he will get increased effects from diminished causes, so it is impossible to think that "alteration of motion" is not "proportional to the motive force impressed." And he maintains that this is, in fact, a latent implicatiion of unconsciously organised experiences just as much as those which the experimenter necessarily postulates.

I may add that if mathematics included in its range the connection between objective phenomena and the answering subjective states, this question would be one for mathematicians; but at present it is, as it seems to me, a question pertaining to the psychological basis of inductive logic. JAMES COLLIER

Bayswater, May 18

The Glacial Period

I THINK there are but few points in Mr. Belt's letter ("The Glacial Period," NATURE, vol. x. p. 25) to which Geologists who have devoted much attention to the ice action will not take exception. May I be allowed to call attention to one or two?

1. I do not believe that there is evidence, which anyone accustomed to glacier "spoor" would admit, of an extension of the ice-cap so far south as the Thames valley.

2. It is in the highest degree improbable that the shells on Moel Tryfaen should have been scooped out of the bed of the North Sea by moving ice and transported to their present position. Apart from the difficulties of a glacier thus walking so far up-hill, and of shells having escaped utter smashing in this uncomfortable mode of transport, Mr. Belt has forgotten that Wales was a centre from which radiated glaciers, and at one time an ice-sheet, which surely would have warded off from its own hills the northern intruder. What evidence is there that the ice-sheet ever followed its path? All that I know points to local glaciation.

3. Mr. Belt forgets that the various sea-marks are often at very different heights above the present water-level—as is so well shown in Scandinavia—and that no lowering of the water will explain this. The height of even 600 ft. which he claims is one that rests on many assumptions and but little confidence can be placed on the numerical results.

It would be easy to discuss many other questions which he raises, but this would occupy far too much space. My present purpose is not so much to do this, as to utter a protest against such a portentous development of a theory which has for some time past been assuming nightmare proportions.

St. John's College, Cambridge, May 19 T. G. BONNEY

Lakes with two Outfalls

IT is quite possible that I am wrong in my memory of the Nystuen watershed; and as Prof. Stanley Jevons examined the place critically, I can have no doubt that I am so. I passed merely as a traveller, and described what I had seen, from a memory, not specially sharpened by a knowledge of the importance of the point, at the time the observation was made. I know well what tricks one's memory plays under such circumstances, particularly when one has been rambling over many similar localities; and my letter indicated that I was in doubt as to the particular lake which gave the double outfall. I passed, too, just after much heavy rain, and it is possible that the boggy bottom which Mr. Jevons describes was temporarily converted into the lake, which deceived me. I may add, that both the guide who brought me over the mountains from Aardal, and the Skydsgudt who took me to Skogstad, confirmed the double outfall.

My object, however, in writing, was chiefly to draw attention to Norway, as offering an admirable field for the settlement of the controversy, without going to the wilds of America. If there be such phenomena, and I believe there are, they may assuredly be looked for in that land of hard granite rock, mountain plateaux, and innumerable watersheds of all sizes and varieties, and if the hundreds of educated Englishmen who go there every year be only impressed with the importance of accurate observations, the point may soon be settled.

Certainly I agree that Colonel Greenwood, who has kindly favoured me with a most interesting letter of advice, has done excellent service by his quite justifiable incredulity, and I shall myself be content to have made a mistake, if by it I shall be the cause of greater accuracy in others. W. B. THELWALL

27, Burghley Road

Glass Cells with Parallel Sides

I SEND you a brief description of a method I have recently employed for rapidly fitting up glass cells with parallel sides, believing that it may be of interest to your readers.

A piece of indiarubber tubing (or of solid rubber) bent into a semicircular form is placed between two equal-sized rectangular plates of glass, the ends of the tube terminating at the upper edges of the glass plates; the plates are then held together by passing two strong indiarubber rings over their ends. If the rings are of such a size as to exert the requisite compression a semicircular water-tight cell is thus obtained, which can be taken to pieces and cleansed with the greatest ease.

A trough so made served well to exhibit with an ordinary magic-lantern the experiments described on pp. 173 and 174 in Tyndall's "Heat a Mode of Motion," and smaller cells suitably fitted with platinum wires, and held in the wooden frame of an ordinary lantern-slide, enabled the galvanic decomposition of acidulated water and of saline solutions to be thrown upon a screen and thus rendered visible to a large audience.

Queenwood College, Hants. FRANK CLOWES

Brilliant Meteor

WHEN nearing Holyhead at 0.50 A.M. on the 19th inst. the most brilliant meteor I have ever seen passed slowly across the heavens. It formed near Antares, remained stationary for two or three seconds, and then slowly moved to the northward, disappearing in the Great Bear. Throughout, the soft green light showed every portion of the hull and rigging with as much distinctness as a number of pyrotechnic fires could have done. The shape was that of an elongated ellipse, slightly contracted at one end, with the major axis of the apparent diameter of the sun. A short time before it disappeared six sparks as large as Jupiter were discharged from the southern end, and I thought a crackling sound followed.

Celtic, May 20 WM. W. KIDDLE

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